Integration Summary Report

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Recent progress in threat definition, detection and search strategies, intercept concepts, experiment definition, and integration studies have produced a preliminary integrated program for detection and defense against the most threatening objects from space. It addresses objects of all sizes and warning times, and could negate the most frequent and damaging impactors with existing, non-threatening, non-nuclear technologies. The appropriate components could be developed and deployed in a few decades for expenditures of a few tens of millions of dollars per year. The program could benefit from interagency and international cooperation in both detection and defense. There are no technological or classification barriers to thus sharing of the burden of planetary defense.

Introduction

Integration is a new panel, whose definition was both necessitated and made possible by the successes of the other panels at the previous meetings. Further progress at this meeting made it possible to achieve the preliminary integration insights discussed below. This section documents the main areas of progress and those in which further work would be useful.

Threat Panel

The Threat Panel estimated that about 2,000 near-Earth objects (NEOs) larger than one km—of which only about 7% have been discovered—revolve around the sun on short-period orbits that can intersect the orbit of the Earth. There is about one chance in a thousand that one of these objects will collide with Earth during an average person's life span. Such a collision could inject sufficient material into the atmosphere to cause a major loss of global crop production and consequent loss of human life. Expected losses in the absence of defenses are incalculable. Even the average expected losses justify expenditures of ≈ $B/yr on defenses and defenses against both NEOs and comets—independent of the current uncertainties in their relative fluxes.

The Threat Panel also refined the estimates of potential losses from the more numerous intermediate sized NEOs with diameters larger than about 0.1 km, which have a chance of about 10 percent per century of colliding with Earth. Their impact in oceans cause tsunami, which can cause great destruction to distances of tens of kilometers inland all around the oceans into which they fall. While the effects from these intermediate few 100 m NEOs are not global, they could be regionally devastating due to the increasing concentration of value in coastal regions. While the losses from still smaller NEOs is not completely characterized, they are apparently of lesser magnitude. Thus, the primary challenge is to design defenses that can address intermediate and large NEOs and comets.

Detection Panel

The Detection Panel adopted a goal of identifying, characterizing, and cataloguing potentially threatening comets and asteroids. For objects larger than 1 km that could be done with a few dedicated 1 to 2 meter telescopes with advanced CCD detector focal planes. Through about 10 years of rapid, wide-area search, they should discover 50-70% of short period NEOs—or about 90%, given strong Air Force and international participation. Additional objects also need study, including long-period comets (LPCs) seen only on their single pass, low albedo "stealth" comets, and intermediate objects that can cause tsunami and enhanced regional damage. The intermediate size objects could require more advanced technology, including space-based sensors, which appear necessary to address their short timelines.

Interdiction Panel

The Interdiction Panel outlined a set of options for each threat object size and warning time. Of particular interest is the conclusion that kinetic energy interception is adequate for a larger class of object than previously thought, including the intermediate size objects causing the most frequent tsunami. Thus, it should be possible to develop the detection, interdiction, sensor, and control technologies needed to address these intermediate objects without invoking threatening technologies while preserving the option to switch to higher specific energy sources later, if needed. Validating these options will require more theory, laboratory experiments, and interaction experiments with space objects.

Experiments Panel

The Experiments Panel outlined a set of steps leading from theory and laboratory experiments to space flyby and probe experiments, which would establish the basis for a mitigation options matrix. They propose a set of carefully diagnosed interaction experiments in space using technology consistent with that required for subsequent applications, which would leave a residual prototype intercept capability. A key element of this program is the international execution of experiments, interpretation of data, and integration with data from other sources—such as that from small objects that impact the Earth's atmosphere. These experiments would culminate in full-scale kinetic energy impact probe experiments, including precise measurements of orbit change with ground- and space-based sensors, which would both study the impulse transferred by kinetic energy impact and simulate the interaction of higher energy sources.

Warning time

In integrating outputs from the Threat, Detection, Interdiction, and Experiments Panels it is useful to subdivide the threat according to the amount of warning time the defense would have. The paragraphs below use the terms Long Warning to indicate the times of decades to centuries that might be available for the negation of NEOs detected many orbits prior to impact, Short Warning to indicate the months to years that might be available for comets or undetected NEOs, and Very Short Warning to denote the days to weeks of warning that might be available for intermediate objects.

Long Warning times result from the detection times of decades to centuries that might be available for the negation of short-period (4-10 years) NEOs detected many orbits prior to impact. For example, detection of a 10 year period NEO 10 orbits prior to impact would give about 100 years for reaction, which would allow considerable development of defensive measures prior to their deployment. Long Warning permits efficient search with conventional telescopes with large CCD arrays. Current and planned ground-based telescopes should be able to provide the search rates required to survey 90% of large NEOs in one to two decades. Space-based sensors should be available for their augmentation, and even more advanced technology should become available for upgrade during the few decade search interval, if needed. Long Warning also makes it possible to take full advantage of efficient interception tools such as optimal trajectories, deflection at perigee, and low-thrust, high specific energy engines and provide enough leverage to take advantage of efficient deflection concepts such as ion thrusters, solar energy, mass drivers, kinetic energy, etc. The highly efficient performance of each of these interception and deflection approaches with Long warning has been made plausible through analytic studies, although they need more detailed systems and engineering studies.

The distinguishing feature of Long Warning is that the most important element of an effective response to this class of NEOs is the prompt initiation of search, which can be performed by modest and inexpensive telescopic searches. If that search found most of the currently unknown short-period NEOs in a few decades and did not find any that represented an immediate threat to the Earth, it might not be necessary to develop means for interception at all. Thus, there appears to be the potential for eliminating risks amounting to a few tens of $B by performing a search which would cost a few $10M. This roughly 100-fold leverage is so great that it would appear cost effective to develop the means for interception even if they were never needed for NEOs in this class. That is particularly so in if they were so constructed that they were applicable to the other classes of objects, which do require defenses.

Short Warning such as the ≈ 4 months that would result from the detection of a LPC or undetected NEO at 2 AU on final approach admit of far fewer search and intercept concepts. Adequate warning against large LPCs requires search to magnitudes of 23 to 24, which requires either massive ground-based telescopes or space-based sensors, either of which could cost ≈ $100M. Reducing the adverse leverage from the object's high velocity would require the high specific impulse and thrust of nuclear fuels, which would be difficult and expensive to develop. Reducing the penalty from the object's great mass would require payloads with very high specific energy, although that would not require expense or development; current versions could suffice. Short Warning requires that defenses be in place when the threat is detected; there would not be time to develop them later. However, if defenses were developed for Short Warning, which appears justified on the basis of the losses from comets alone, they would also be available to Long Warning threats, although the converse is not true.

Very short warning also requires ready defenses. It needs very fast, wide area search, independent of solar viewing and weather restrictions, for which space basing provides further leverage. For intermediate-size objects, the sensor requirements are not excessive. A ≈ 1 m sensor, which could detect a 1 km object at ≈ 1 AU, could also detect a ≈ 0.1 km object at ≈ 0.1 AU, which would give a warning of about a week. It would not be possible to deflect such an object totally away from the Earth with that little warning, but it should be possible to put a kinetic energy payload in its path that could disrupt it sufficiently such that the Earth's atmosphere could deal with the residue. For intermediate objects, this kinetic energy defense could be developed through a modest number of experiments, whose most stressing elements would be the development of quick-response rockets, homing technology, and control technologies. Should an target require more energy than kinetic energy could provide, it should be possible to substitute more energetic means into the interceptor without invalidating these experimental results or requiring revalidation of the defenses.

Future activities

Future activities include studies, interaction experiments, integration experiments, and operations. Studies should cover the detailed search, interception, and deflection technologies discussed above and define their integrated performance at the level required to estimate their performance and cost. To support this, there is an immediate need for systems concept definition, mission analyses, system engineering, and technology studies to guide the follow on phases. Given adequate definition of the candidate defensive system, various professional societies could make important contributions and add credibility to these assessments. An important part of this activity would be the proper assessment and documentation of expected losses, e.g., a handbook of the likely effects of and expected losses from tsunami in various basins. These studies could be an important measure in maintaining communication among active workers and in initiate educational efforts in the field of NEO defenses. This study phase should cover a few years and could be executed at a funding level of a few $M/yr.

The experiments phase should involve both laboratory and then space experiments. The laboratory experiments should be as thorough as possible, and should test the micro- and macro-mechanical properties of a wide range of candidate objects. These experiments could profitably overlap the study phase somewhat, as they would thereby provide useful focus and guidance. The space activities, which should progress through flyby, probe, and deflection experiments supported and diagnosed by ground-based sensors, could build on the successful Clementine technology, approach, and cost structure. If so, the experimental programs could probably be executed for a few $10M/yr. These space experiments should be thorough, as any defenses should be based on knowledge of the NEO’s characteristics.

To that end, international contributions could be very useful in adding unique diagnostic techniques and in integrating U.S. strength in search, homing, and impact technologies with complementary international strengths in rockets, timely response systems, and space science. These space capabilities, although pivotal, are now fragile and possibly transient. However, it might be possible to augment them through the use of deactivated strategic missiles for experiments, if the value of that option could be agreed to quickly. These experiments would also provide a mechanism for practicing through joint field activities the coordination of command and control procedures that could later be useful for actual defenses. The value of these activities would be enhanced if the data could be transferred to an international center for data analysis, which could in time evolve into a joint warning center for NEO hazards.

Integration experiments could overlap the space interaction experiments somewhat, as they would be intended to test the integrated performance of the search, interaction, and command elements, all of which would be needed for precise, properly diagnosed interaction experiments. If this synergism was exploited properly, the integration experiments could be performed for an amount comparable to that for the interaction experiments. Subsequent to these development programs, the residual intercept capability could be maintained for about $100M/yr, with the greater amount resulting from continuous operations and higher reliability, whose timelines and costs would be paced accordingly.

Strawman programs

To provide interim answers to frequent questions as to when it might be possible to develop actual defenses against various space object threats it is useful to sketch out strawman programs that would meet the timelines for the various objects discussed above. The strawman program for Long Warning is shown below:

Long	95	00	05	10	15
search	____________________________		50%	90%__________	
intercept	current___________________	maintain____________			
improved	study_____|_______________	develop____________|			

The three main elements of the program are search, intercept, and improved technology. Adequate search could apparently be performed by one to two, few meter ground-based telescopes with upgraded CCD focal planes. They should achieve 50% completeness against short-period NEOs in about a decade and over 90% in two. The completion of this search is the pacing item for defenses against objects in this class, and the pacing item for completing the search is getting it started quickly with modest telescopes with good focal planes. The interceptors that would be used in the near term are slight modifications of current deep-space missiles. The improved technology development indicated would involve modified boosters, efficient upper stages, improved deflection means, and more accurate control technologies. After the initial study, key concepts and components could be developed further, although it would not be necessary to integrate them before the results of the telescopic search was known, which is why the intercept programs are paced to the search program.

For Short Warning, a few additional program elements must be added to address the much deeper and faster search needed and the need for ready defenses and the experiments required to support them. The resulting strawman program is:

Short	95	00	05	10	15
search	study____________|	______develop____________| operate			
" space	study_ __________|	______develop_________________________		deploy	
interact exp.	plan _ __________|	______execute_____________| complete			
intercept tech	plan_____________|	______test________________|	_______residual		

The key elements are the need to develop much more powerful ground- or space-based sensors. The former would build on, but represent a major extension of, the technology envisioned for near-term, decades long telescopic searches. The latter would involve the development of new search technologies with performance significantly beyond that currently available for observation from space. For that reason, the space-based sensors could take longer to develop and be available later somewhat later. The interaction experiments are paced by the space experiments needed on flyby observables, probes, and kinetic energy deflection. Fortunately, they could be performed with existing or surplus assets. The interception technology experiments could largely be performed as the control mechanisms for those interaction experiments, which would speed the execution and lower the cost of each. These interaction experiments with kinetic energy should also adequately simulate the coupling of very high energy explosives as well. The value of both sets of experiments would be enhanced in both their execution and interpretation if they could be performed with international cooperation as full as possible.

For Very Short Warning, the strawman program is similar, although some of the lines have slightly different interpretations:

Very Short	95	00	05	10	15
search ground	plan_____________|	___________| modify			
" space	plan_____________|	___________| modify			
interact exp.	plan_____________|	___________| complete			
intercept tech	plan _____________|	___________| residual			

The distinction is that this program addresses intermediate size NEOs; hence, it can use components that can work with less response time because complete deflection or fragmentation is not required. The Earth's atmosphere can provide some of the defense. The requirement is that the object be broken up and dispersed enough so that the fragments would not survive in enough size and number to coherently produce a tsunami. For such a system ground-based sensors derived from those for Long warning search (e.g., more 1 m telescopes with focal planes that would support an order of magnitude greater search rates) could provide adequate warning, although modest space-based sensors could be developed rapidly to extend search closer to the sun and as a backup to terrestrial weather interruption of ground-based search. These simplifications should enable these modest sensors to be developed significantly more quickly and with less expense than those for Short Warning. The interaction experiments would be similar to those for Short Warning, they would just be done faster, which should not involve technological issues, just modest augmentation of resources. Similarly, the intercept technology could easily be accelerated to complete all of the required experiments required to reach the desired residual intercept capability sooner. The net effect is about a five year acceleration of the strawman program for Very Short Warning compared to that for Long or Short

Warning, which largely stems from the reduced levels of performance required from search sensors, the simplicity of the negation concepts, and the direct applicability of existing missile and intercept technology.

Strawman resource requirements

The strawman programs above appear technically reasonable, but it is appropriate to provide some indication of the resources assumed in constructing them. This section provides a strawman estimate of the resources required for each program and a rough cross comparison of them.

For *Long Warning*, the three main elements of the program would be started in parallel. The search program would involve the several, few-meter telescope program outlined above. Achieving the 50 and 90% completeness levels shown is estimated to cost about $5M/yr, although the times for reaching the latter could be accelerated about a factor of two by funding of about twice that amount. The intercept program is a modest one of studying and modifying current rockets for more accurate deep-space rendezvous, which would also cost about $5M/yr. The element for the development of improved technology for boosters, deflection, and control would shift at the five year point from studies to technology demonstrations and experiments, which would cost somewhat over $5M/yr. It would not be useful to accelerate the intercept and technology programs without accelerating the search program as well. Together the three elements as shown would require about $15M/yr, or a total of ≈ $225M over the 15 year development program shown. The $15M/yr would be about 3% of the expected annual losses from NEOs in this class. Accelerating the program to $30M/yr would not improve its cost-effectiveness during the development phase, but would speed the date at which actual defenses would be provided.

For *Short Warning*, there is an essential requirement to develop much more powerful sensors. Since it is not clear whether ground- or space-based sensors would be required, it would be appropriate to start their development in parallel. For ground-based sensors, that could probably be done as an extension of current telescopic approaches for an additional ≈ $5M/yr, for a total of ≈ $10M/yr for ground-based sensors. The conventional wisdom is that space-based sensors would cost an additional ≈ $20M/yr and take another 5 to 10 years for development. Although current concepts challenge that wisdom, those numbers are used below. The series of progressive space interaction experiments needed could be performed in about 15 years for about $30M/yr by building on the technology demonstrated in Clementine, which would make their results available at about the same time as the sensor developments and the integration experiments that would use them. Those interaction experiments could also be used to test interception technology for an additional ≈ $20M/yr. That would give a total requirement for the Short Warning program of ≈ $80M/yr, or $1.2B over 15 years, which is $1.2B/ ($0.5B/yr x 15 yr) = 16% of the losses expected from objects in this class over that interval. It should be noted that while the Long Warning strawman plan would produce only a survey at the end of 15 years, the Short Warning strawman would produce a significant residual defensive capability. However, because of the long development times for the sensors for Short warning, it is more difficult effectively accelerate its timelines.

For *Very Short Warning*, it is necessary to upgrade and deploy more ground-based telescopes, although they could be modest sensors at the GEODSS level, which are readily available, which would cost about $10M/yr. It would also be appropriate to augment them by deploying modest space telescopes for greater coverage at angles closer to the sun, which might require an additional $20M/yr. Since those two steps would accelerate the completion of the sensor elements to about a decade, it would be appropriate to do the same with the interaction and interception technology experiments, although that should not add appreciably to their costs, which would remain at ≈ $30M/yr and $20M/yr, respectively. This acceleration would produce a defensive capability against intermediate objects about five years earlier than the other programs for an investment of about $80M/yr x 10 years ≈ $800M, which is about half the expected losses from intermediate objects during that interval. Because of the development times for sensors and execution times for interaction and integration experiments, it would be difficult to further accelerate these timelines. However, these defenses, once developed would be available to support the detections from other warning times.

Overall, Long Warning would only lead to an assessment after 15-20 years, not a defense. Since it does not lead to a defense, it can be argued that search only would be about as effective, as has been assumed in earlier studies of objects in this class. A program for Long Warning is the lowest cost option, and does address a significant part of the threat, so it should not be excluded. A program for Short Warning addresses the portion of the threat that cannot be treated by telescopic searches of extended duration. It handles the other global part of the threat, but because of the sensor development required, it does so on a long time scale and at considerable expense. A program for Very Short Warning would lead to both search and defense for regionally threatening intermediate objects. It would also lead to a defensive capability and backup for objects from longer warning searches. The cost for all three of ≈ $175M/yr would be cost effective relative to the threat, but would constitute a significant increment. Thus, it is useful to examine various combinations that could be undertaken at lower annual costs.

In evaluating combinations, it has been conventional to first examine expanding the program for extended search to Long Warning and second to extend it to include Short Warning defenses. Since Very Short Warning defenses against intermediate objects are the newest concern, there is a tendency to treat them as third place. Given the arguments and estimates above, it is not clear that is the appropriate path. Extending to defense with Long Warning is clearly appropriate, because it is a modest extension and that is highly cost effective. But based on the timelines and technology developments outlined above, it would appear that the second step should be defenses against Very Short Warning intermediate sized objects, because that would provide an early capability against the threats that are likely to be encountered first. Executing the Very Short Warning program would start towards the improved ground- and space-based sensors needed for other warning regimes, the limited interaction experiments needed for non-threatening technologies, and adequate demonstration of intercept technology integration. Executing the programs for Long and Very Short Warning defenses together would only cost about $100M/yr, but it would address both the large objects that can be detected by extended search and the intermediate objects that can be negated on final approach. If Short Warning defense was added later, it could build on the technology developments and experiments performed by the two earlier programs to produce defenses using longer ranges and greater specific energies to negate larger objects.

Summary

Integration is a new function made possible by the success of previous meetings in defining the status of the threat, detection, interdiction, and supporting experiments. Further progress at this meeting made it possible to achieve the insights discussed above. The Threat Panel found there was a significant threat from both intermediate and large objects. The Detection Panel indicated means for detecting each of them. The Interdiction Panel outlined options for addressing each as a function of warning time. The Experiments Panel outlined the laboratory and space experiments needed to validate those predictions. The products of those four panels provide an adequate set of tools for negating each class of threat objects, which vary depending whether the object is detected with Long, Short, or Very Short Warning. For Long Warning there are many options for search and negation based on current technology. As warning is reduced, so are the options. However, they remain adequate to span significant and important parts of the threat with non-threatening technologies, which have the capacity for growth to address much larger objects.

These predictions will have to be confirmed through further studies, but if they remain positive, it should be possible to define affordable laboratory and space experiments to demonstrate the effectiveness and affordability of these defenses. These experiments could be performed within one to two decades and could result in a residual capacity for the interception of an important class of intermediate size objects and in the development of interceptor and control capabilities that could support negation technologies of any required energy.

The programs for defensive alternatives depend most strongly on the sizes of threat objects and the warning time they permit. Long Warning programs for objects observed many orbits prior to impact have been studied extensively, but do not lead to actual defenses and address only a portion of the threat. Short Warning programs remedy those deficiencies, but require significant technology development, cost, and delay. Very Short Warning programs could produce early defenses against the most likely threats, which they could address with non threatening technologies with growth potential to objects of all sizes. Executing the Very Short Warning program would move towards the improved ground- and space-based sensors needed for other warning regimes, the limited interaction experiments needed for non-threatening technologies, and adequate demonstration of intercept technology integration. Executing the Long and Very Short Warning programs together would cost about $100M/yr, but would address both the large objects that can be detected by extended search and the intermediate objects that can be negated on final approach with defenses that could build on these technology developments and experiments to produce defenses against larger objects.

It is clear that the threat from both intermediate and large space objects exists. It appears that adequate technology to search for and intercept them exists or can be built on current technology. It also appears that straightforward experiments could be performed to test these technologies and find out the performance and effectiveness of these defenses relative to the expected losses from the impact of these objects. On the basis of preliminary studies, it appears that a combination of search and defenses is more effective than detection alone. A modest combination of studies, laboratory and space experiments, and intercept technology developments, which could be performed openly with scientific and international cooperation, could refine those estimates and objectively assess the ultimate effectiveness and affordability of the full range of defenses needed. There are excellent opportunities for international collaboration in this assessment, and in the testing and deployment of these capabilities, if the formulation and initiation of a responsive program is addressed soon.